

## 1. In the Beginning

**1.1. Previous Experience:** I joined the staff at the Livermore Campus of the UC Rad Lab in August of 1956. Though the Campus was only four years old, they had already developed a number of computer codes for various purposes relevant to weapons design. During the late 1950's, I worked with Hugh Smith to merge these codes into LLNL's "all purpose" one-dimensional hydrodynamics and energy transport code. I presume this experience is what drew Stirling Colgate to my office, in 1959, with a proposal to apply the numerical methods to an interesting astrophysics problem.

**1.2. Colgate Proposes a Problem:** Stir's proposal was not to study core collapse supernovae. Rather, he was interested in studying the shock wave generated by a supernova explosion. He argued that the shock would strengthen as it proceeded through the steep negative density gradient and ultimately, at least for big supernovae, would reach relativistic velocities. The blow off from this shock, he hypothesized, was the source of cosmic rays. We now accept this scenario as an important source of relativistic primary cosmic rays but, in 1959, it was a fresh idea. I was immediately taken in by Stir's enthusiasm and excited by the prospect of applying our expertise in numerical hydrodynamics, and the Livermore Lab's unparalleled computing power, to scientific research.

## 2. Our Resources

**2.1. Computers:** When I first arrived at Livermore, the IBM 701's had just been replaced by 704's. These were state of the art machines at that time. Not much by today's standards. The cycle time was 12  $\mu$ s (83000 Hz); a floating point multiply required 17 cycles ( $\sim$  5000 floating point multiples per second). RAM was  $\sim 10^5$  Bytes: puny by comparison with my current laptop's frequency of 2.6 GHz and memory measured in GBytes. Programming often required a tradeoff between memory and speed. Nonetheless, these machines were crucial to tackling the supernova explosion and shock propagation problem. The limited memory restricted the zoning and affected the quality of the calculations. During the course of our research, machines were continually upgraded. We progressed from the 704s to the 709s, 7090s, 7094s, CDC 3600s, to the CDC 7600s in the space of about six years. With each upgrade we were able to refine the zoning and cut the compute time. (Other machines were developed and used at Livermore during this period, e.g. LARC, STRETCH, CDC 1604, but we didn't run on these.) Another important development during this period was the ability to produce graphical output.

**2.2. Existing Code:** Livermore's existing one-dimensional hydrodynamics and energy transport code could be modified to include the required gravitational fields. Those who have written big codes will appreciate the advantage of having an existing code to work with: most of the coding is bookkeeping: managing print outs, tape dumps, graphics, input, and other tasks not directly related to  $F=m a$ .

**2.3. B<sup>2</sup>FH:** The justly famous paper by Geoff & Margret Burbidge, William Fowler, and Fred Hoyle titled "Synthesis of the Elements in Stars" (Reviews.

of Modern Physics, 1957); affectionately known as B<sup>2</sup>FH, provided a model for initiating the supernova explosion.

**2.4. Bill Grasberger** provided equation of state data for the dense iron pre-supernova stellar core and the iron to helium transition and later for the subsequent transition to protons and electrons.

**2.5. Edward Teller** lent his influence and support to the project. Though Edward was not the Laboratory Director (except for a very brief period circa 1969), his influence was substantial, close to “whatever Edward wants, Edward gets.” He had a great interest in the shock problem as it related to nuclear synthesis. He played a behind the scenes role, as a critic, and by helping us to procure needed computer time.

### **3. How We Proceeded**

#### **3.1. Modify Code to Include Newtonian Gravity**

**3.2. Check Code’s Solutions Against Analytic Solutions:** We put the code through a battery of tests to verify its capabilities. Tests were selected to check four types of problems important to stellar hydrodynamics: Equilibrium of a polytropic configuration, Free Fall, Adiabatic Expansion through a density change by factor of  $10^{20}$ , and Shock Propagation. We compared our numerical results with known analytic solutions.

#### **3.3. Install Grasberger EOS**

**3.4. Establishing an Equilibrium Configuration:** The tests for equilibrium highlighted Stirling’s knack for focusing on the relevant physics. He often came up with quick fixes based upon his keen physical intuition. In the equilibrium test, we fed the code initial data taken from an analytic solution for a polytrope. Since the numerical representation of the physics contained inevitable truncation errors, the configuration oscillated. The oscillations were small and very slowly damped; they were annoying and could prematurely initiate the conversion of iron to helium in isolated zones. We aimed to start from a quiescent pre-collapse configuration. Stirling came up with a solution: He recognized that the oscillations were simply sound waves bouncing around and slowly dissipating their energy as the configuration sought the equilibrium configuration. We sped the equilibration by multiplying the artificial viscosity by a large factor. This had the effect of dramatically hastening the dissipation of the energy in the compression waves but did not change the final equilibrium state. It worked like a charm; we quickly produced quiescent configurations that closely approximated the analytic polytrope solutions. Later, I developed an algorithm that inverted the difference equations and solved for the mass density distribution that produced exact equilibrium in the difference approximation

**3.5. Early Results:** Upon completing our tests we set up a plausible quiescent initial configuration, a 10 solar mass polytrope, and introduced a small energy sink to simulate the evolution to nudge the core density into the region where the iron to helium transition would occur. The initial collapse followed the scenario of B<sup>2</sup>FH. We expected (as per B<sup>2</sup>FH) that once the helium core developed, the sudden increase in the pressure gradient would stop the collapse and send a compression wave outward from the core. This wave

would trigger thermonuclear reactions in the material surrounding the core. Though the B<sup>2</sup>FH scenario paralleled the mechanisms we associate with Type I supernova today, our first calculations of our 10 solar mass star failed to develop a strong compression wave. Furthermore, the simulation of a thermonuclear interaction by depositing energy in the shells surrounding the core produced an explosion directed predominantly inward and so enhanced the collapse. With or without the simulated thermonuclear reactions, the core density became so large that the helium core would be expected to fission into protons and electrons. This departure from the B<sup>2</sup>FH scenario was not at all what we had expected.

#### **4. Back to the Drawing Board**

- 4.1. Extended Grasberger EOS:** We prevailed upon Bill Grasberger to extend his EOS tabulation to include the break up of the Helium into Hydrogen and continued the calculations. The collapse took us to densities where we knew the electrons would be pressed into the protons, converting the core to neutrons. Again Grasberger came to our aid.
- 4.2. Nucleon Hardcore Potential:** When the core reached densities near that of nuclear matter, we modified the EOS to simulate a Nucleon Hardcore Potential. The collapse halted when the core reached nuclear densities and the expected bounce occurred; a shock swept outward.
- 4.3. Shock Propagation:** Calculation of the shock met with difficulties due to coarse zoning mandated by limited computer memory. Time steps were automatically cut to preserve accuracy. A single collapse calculation took many hours on the 704-709 series computers. Computer time was at a premium as the Lab's computers ran 24-7 and weapons calculations had priority. At one point we were struggling to meet a deadline for presentation of our results to a conference. We just didn't have enough time to finish before the deadline. Edward Teller stepped in and arranged for us to commandeer one 709. I camped out in the machine room, napping on the floor, and brown bagging my meals, to shepherd the calculation to completion.
- 4.4. Neutrinos:** Ultimately our first calculations had difficulties now familiar to all supernovae modelers: Infalling material stalled the shock. We were perplexed until a back of the envelope calculation, using the neutrino cross section first measured by Reines and Cowan four years earlier, indicated there would be significant neutrino deposition in the infalling material.

We contrived a crude model for neutrino deposition and were then successful in generating a "secondary" shock that blew off the outer layers of the star. Within about ten years, using more sophisticated codes, a better understanding of neutrinos, and significantly larger and faster computers, Jim Wilson, Stan Woosley, and others alternately confirmed and denied that neutrino deposition is adequate to blow the lid off the collapsed star. It is now accepted that turbulence (requiring two or three dimensional models) also plays an important role in the explosion. In any case, it is generally agreed that, as our model predicted, a core collapse supernova generates a

humongous fluence of anti-neutrinos and that neutrino transport plays a crucial role in moving energy out of the core's deep gravitational well.

## **5. Relativistic Code**

**5.1. The Road to General Relativity:** A check of one of our collapsed cores revealed that relativistic corrections would increase the gravitational effects by about 20%. This raised the possibility that the core collapse would not be arrested. To put this in context, though Chandrasekhar (and others) had previously shown that stars above a critical mass could not be supported on degeneracy pressure, virtually no one took seriously the possibility that collapse could lead to a singularity. Though unsupported by any rigorous argument, it was commonly held that mass ejection would bring the star's mass below the critical limit. Singularities were an anathema that could not be tolerated. Nonetheless, John Wheeler, who frequently visited Livermore as a consultant, took a great interest in our calculations and several times returned to Princeton with copies of our print outs which he shared with his students.

**5.2. Symposium 1964:** During the summer of 1964 we arranged for a symposium on GR astrophysics to be held at Livermore. Though Colgate had already decided to leave Livermore to become President of "New Mexico Tech", he was very much the driving force behind the Symposium. As I best recall, the symposium lasted for about a month, with interested participants coming and going throughout that period. Attendees included John Wheeler, Charlie Misner, Kip Thorne, David Sharp, Richard Lindquist, Jim Bardeen, Al Cameron, and others, and, of course, Colgate and myself. Several important papers came out of the discussions.

**5.3. Formulation of the Code:** My own focus was on developing a GR version of the code used in the supernova calculations. I worked closely with Charlie Misner to develop a method paralleling the method used in the Newtonian code. This focused on the conservation laws rather than on the Einstein Equations and formulated those laws in a Lagrange coordinate frame. This approach allowed us to directly compare the Newtonian and GR calculations. I recall Charlie educating me to the advantages of focusing on the fluid's 4-velocity rather than the 3-velocity. Following the symposium, Charlie and I continued corresponding by mail. He and David Sharp published their version of the Lagrange-frame equations before the year ended. My own version differed from the Misner-Sharp formulation in that they assumed conservation of baryons and used baryon number as the "radial" coordinate; I opted to use the rest mass as a radial coordinate, just as was done in the Newtonian code. So long as rest mass and baryon number are conserved, the two methods are consistent.

Following Stir's departure for Socorro, I continued work on the GR version of the code. Mike May, who had been on the Livermore staff, then left to join a southern California think tank, returned to Livermore. He indicated an interest in collaborating with me on the GR calculations. We developed a good

working relationship and within a couple of months had a running but untested version of the GR code.

**5.4. Second Texas Symposium on GR Astrophysics:** This symposium was held in December 1964 in Austin, Texas. The symposium focused mainly upon the mystery of Quasars. At this time, it was not yet established whether they are very distant objects or, possibly objects within our own galaxy. At that time, they were known only as very powerful radio sources; their positions in the sky were not yet well defined and their distances from Earth were open to debate. Alan Sandage presented his study of what he called "blue stellar objects". He found these optical sources were consistently paired with the Quasar radio frequency sources and cautiously opined that they were the source of the Quasars. Burbidge and Fowler presented one of their several speculative papers attempting to reconcile the immense luminosities of the Quasars with reasonable models.

Though the meeting focused primarily upon Quasars, Colgate and Wheeler arranged a session on gravitational collapse. I presented our first relativistic calculations of core collapse, which had been completed just days before the symposium began. These calculations showed unexplained wild oscillations developing as the collapse proceeded. I was careful to caution that these likely were of numerical origin and should not be taken seriously until we had a chance to review the calculations. Once back in Livermore, I discovered that the oscillations resulted from the form of the artificial viscosity ( $Q$ ) we used: a relativistic version of the Von-Neumann-Richtmeyer form. At first I simply shut off the  $Q$  during the collapse phase of the calculation; later I replaced the  $Q$  with a form better suited to the physics of the collapsing star. In retrospect, I think that the oscillations showed the sensitivity of the velocity of the collapse to small perturbations and could have been a clue that convective turbulence (not included in our 1-D model) plays an important role in transport of energy within the supernova core.

Two amusing memories from the 2<sup>nd</sup> Texas Symposium:

Playing Tennis with Colgate as the "norther" blew in

Fowler on the Airplane with Wheeler, Misner, Chandrasekhar, Maarten Schmidt, Alan Sandage, Geoff Burbidge...

**5.5. Results from GR Code:** We studied the collapse of stars with masses ranging from 2.1 to 210 solar masses and confirmed that large mass stars may enter a state of what we called "continued gravitational collapse". We followed this collapse beyond where the core fell inside its own Schwarzschild radius. Contemporaneous with our calculations. Hernandez and Misner used a theorem (also contemporaneous) from Roger Penrose to show that inevitably this led to a singularity. We followed the collapse right up to the point that the singularity appeared, forcing us to terminate the calculation. It took the genius of John Wheeler to come up with the name "black hole" that propelled the phenomenon into the popular vocabulary.

## 6. Publications:

**6.1. Supernovae:** We published several reports on the supernova calculations; our final joint publication, summarizing all the work, was published as a UCRL report in July of 1964. We submitted the manuscript to the *Astrophysical Journal*. After a very long period, the reviewer returned an uninformative and brief critique and recommended that the paper not be published. Chandrasekhar (who was the editor of the journal) overruled the reviewer and accepted the paper for publication. When Arthur Miller was writing his biography of Chandrasekhar ("Empire of the Stars"), he interviewed Stir who, with his typical generosity in giving credit to others, told Miller that Chandrasekhar had done extensive editing on the manuscript. Though I knew that there were lengthy discussions with Chandrasekhar, I told Miller that "extensive editing" was at odds with my own memory. I volunteered to compare the final published version with the 1964 UCRL report. After a painstaking paragraph-by-paragraph comparison, I found less than a dozen modifications in the 55 page long text; all were of a trivial nature. The paper was published in May of 1966 under the title "The Hydrodynamic Behavior of Supernovae Explosions". Ironically, though originally rejected by the reviewer, the American Astronomical Society selected the paper to be republished in the year 2000 special edition of the *Astrophysical Journal* celebrating the Society's centennial.

**6.2. Black Holes:** Results from the GR code were published in the *Physical Review* in January 1966 under the title "Calculations of General-Relativistic Collapse". It appeared about 5 months prior to the supernova paper, though the supernova work was completed in 1964, the year we began work on the GR code. A detailed description of the code and test calculations appeared as a chapter in "Methods in Computational Physics", volume 7 (1967).

## 7. Socorro 1967

**7.1. Yet Another Code:** Stir invited me to visit him in Socorro where I spent two weeks writing and debugging a 1-D Newtonian hydrodynamics and radiation transport code, oriented toward astrophysics.

**7.2. The Lightning Machine:** As usual, Stir had multiple projects going. One of these, quintessentially Colgate, was inspired by the spectacular daily lightning displays in the nearby Magdalena Mountains: he was building a lightning machine, an adaptation of the Van de Graaff generator. This consisted of an aircraft engine mounted to face upward; an array of "brushes" sprayed charge to electrify the exhaust which was blown into the atmosphere to create a charged cloud. The machine was still in development during my visit and I don't know if he was ever successful at creating a bolt of lightning.

**7.3. Another Project:** The College's Presidential Residence was a suite over one of the labs, a bit austere and perhaps inappropriate for entertaining guests of the President. He and Rosie designed and built a snazzy President's retreat at the edge of the college's golf course. They called it "The Pad"; it comprised a small kitchen, a large living room area, a spacious bedroom, and a changing room for guests who used the swimming pool. The kids (Hank,

Arthur, and Sara), then in their teens, took over the quarters over the lab. Stir and Rosie occupied the Pad.

8. **SN 1987A:** Early in 1987 a supernova appeared in the skies over Earth's Southern Hemisphere. Several neutrino detectors, designed primarily to detect solar and cosmic ray neutrinos, were in operation. Three separate detectors located in Japan, Russia, and the USA reported simultaneous bursts of anti-neutrinos that were likely associated with the supernova event. Though only 24 anti-neutrinos were observed worldwide, they confirmed our prediction that supernovae produce huge neutrino fluence. I can hardly describe the excitement this aroused in Stir and I. At 168,000 light years, SN1987A was the first SN to occur close enough to Earth to produce a measurable fluence of neutrinos in nearly 400 years. Galileo reported the previous "nearby" event in 1604. Considering the rarity of these events in our galaxy, we had no reasonable expectation that our predictions viz a viz neutrinos would be verified during our lifetimes.

9. **A Final Story Concerning Colgate, Teller, and Wheeler**

10. **A funny story about Stirling concerning White & Fowler**

Dear Richard,

The first tale requires little background except that it occurred during the Second Texas Symposium on Relativistic Astrophysics held in Austin TX in December 1964:

One noontime Stir managed to borrow a couple of rackets and some tennis balls. We set out to play a couple of sets at around noon time. Though the calendar said winter, the temperature hovered around 80 degrees. We played one set and started a second when a north wind hit us. At first it was just a bit gusty and we played on. But then, I recall throwing the ball up for a serve just as a strong blast of wind hit and carried the ball several feet to my right, out of my reach. On a second try, I got the serve off but the wind blew the ball in a wide arc and Stir (exhibiting his usual exuberance) fell on his face stretching but failing to reach it. We decided it was time to stop. Within several hours, the wind became a steady chilly blast. The temperature dropped to near freezing. I'd never experienced such a radical change in the weather in so short a time. I inquired of one of the locals whether this was a frequent occurrence. He replied "Oh, that's just a Norther comin' in." His tone of voice and facial expression as he uttered the Texas word "Norther" suggested that it was some of that damn Yankee weather coming along to spoil the day.

The second story doesn't involve Stir but it gives a taste of Willie Fowler's notorious elfish sense of humor. It requires a little background which I've copied out of the notes I previously

sent you:

The Symposium focused mainly upon the mystery of Quasars. (discussed by Maarten Schmidt in 1963) At the time, it was not yet established whether they are very distant objects or, possibly, objects within our own galaxy even as close as the edge of the solar system. They were known only as very powerful radio sources; their positions in the sky were not yet well defined and their distances from Earth were open to debate. Alan Sandage presented his study of what he called "Blue Stellar Objects". He found these optical sources were consistently paired with the Quasar radio frequency sources and cautiously opined that they were the source of the Quasars' radio frequency emissions. Burbidge and Fowler presented one of their several speculative papers attempting to reconcile the luminosities of the Quasars with reasonable models.

When the Symposium ended, most of the participants boarded a plane to Waco and then on to Dallas. The plane, a small, two engine prop driven model (DC3?) accommodated 30 to 40 passengers. Virtually all the passengers were participants from the conference. There were two rows of two seats. I sat in a window seat, approximately mid-plane. Willie Fowler had the aisle seat next to me.

Soon after take off, when the seat belt lights went off, Fowler stood up, stepped into the aisle, placed his hand over his brow as if to shield his eyes from a blinding light. He faced forward where sat John Wheeler, Charlie Misner, Alan Sandage, Maarten Schmidt and others. Fowler, still shielding his eyes, turned to look directly across the aisle to see Chandrasekhar. Then he turned again, to face the rear of the plane, viewing Geoff and Margaret Burbidge, Jim Bardeen, Fred Hoyle, Walter Sullivan (science writer and editor for the NY Times) and others that I don't recall. Then he proclaimed in a voice that could be heard throughout the aircraft: "If this plane were to crash, we could get a new start on this Quasar problem."